LETTERS TO THE EDITOR.

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The Stresses in Masonry Dams.

IF Prof. E. Brown will refer again to my letter published in NATURE of January 2, he will see that I specifically stated that I did not question that in so far as the dam acted as a horizontal beam, the stresses $\widehat{xx}$ and $\widehat{zz}$ will be different in an actual dam and a slab dam. The real point is that Prof. Karl Pearson has said that, apart from any action of this kind, the stresses $\widehat{xx}$ and $\widehat{zz}$ in a slab dam and in an actual dam are widely different. Thus, on p. 11 of Pearson and Pollard's paper, he says Messrs. Wilson and Gore put the stress $\widehat{yy}$ zero, and "hence the vertical and horizontal pressures they calculated have no direct application to real dams." He has since explained in *Engineering*, September 20, 1907, that he did not here refer to any action of the kind mentioned by Prof. Brown and by myself in my original letter to you, but that, apart from any action of this kind, the stresses $\widehat{xx}$ and $\widehat{zz}$ are entirely different in a slab dam and a long dam. Messrs. Wilson and Gore deduced the stresses for their slab by the equations

$$\widehat{xx} = \frac{E}{\left(1 - \frac{1}{m^2}\right)}\left(e_1 + \frac{e_2}{m}\right)$$

$$\widehat{zz} = \frac{E}{\left(1 - \frac{1}{m^2}\right)}\left(e_2 + \frac{e_1}{m}\right),$$

where E is Young's modulus, e_1 and e_2 the measured strains, and m Poisson's ratio. So far as I can see, these equations give correctly the stresses $\widehat{xx}$ and $\widehat{zz}$ for the slab, and if my reasoning in my previous letter to you is correct, these stresses will be unaltered when the slab forms part of a complete dam and is then exposed to stresses $\widehat{yy}$ at right angles to the plane of the other two.

H. M. MARTIN.

83 St. James's Road, Croydon, January 10.

MAY I ask whether the interesting experiments on gelatin models of masonry dams, recorded in your issue for January 2, p. 209, do not ignore one factor on which the stability of actual dams is calculated largely to depend, namely, the weight of its materials?

This factor does not seem to be reproduced in the model, and I conceive that it may account for the rupture depicted on p. 210, which is hardly of a kind that one would expect to have to guard against in a real dam.

OLIVER LODGE.

MR. MARTIN in his letter to NATURE of January 2 cites the body stress-equations of elasticity

$$\left.\begin{aligned} \frac{a\widehat{xx}}{dx} + \frac{d\widehat{xz}}{az} &= 0 \\ \frac{d\widehat{xz}}{dx} + \frac{d\widehat{zz}}{dz} + \rho g &= 0 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad \text{(i)}$$

and appears entirely to overlook the fact that two equations will not suffice to determine the *three* stresses $\widehat{xx}$, $\widehat{zz}$, and $\widehat{xz}$. There is another relation between the stresses, and this relation depends on the relation between the strains, which is purely geometrical, namely,

$$\frac{d^2\sigma_{xz}}{dxdz} = \frac{d^2 s_x}{dz^2} + \frac{d^2 s_z}{dx^2} \quad . \quad . \quad . \quad . \quad \text{(ii)}$$

In substituting for these strains in terms of the stresses the resulting third equation differs for the cases of the dam and of the slab.

For the case of the dam abutting at its terminals against rigid supports:—

$$4\frac{d^2\widehat{xz}}{dxdz} = 2\eta\left(\frac{d^2}{dx^2} + \frac{d^2}{dz^2}\right)\left(\widehat{xx} + \widehat{zz}\right) + \left(\frac{d^2}{dz^2} - \frac{d^2}{dx^2}\right)\left(\widehat{xx} - \widehat{zz}\right) \text{(iii)}$$

For the slab with free faces:—

$$4\frac{d^2\widehat{xz}}{dxdz} = \frac{1 + 2\eta}{3 - 2\eta}\left(\frac{d^2}{dx^2} + \frac{d^2}{dz^2}\right)\left(\widehat{xx} + \widehat{zz}\right) + \left(\frac{d^2}{dz^2} - \frac{d^2}{dx^2}\right)\left(\widehat{xx} - \widehat{zz}\right) \quad . \quad . \quad . \quad \text{(iv)}$$

where η is Poisson's ratio.

Messrs. Wilson and Gore have determined their $\widehat{xx}$, $\widehat{zz}$, and $\widehat{xz}$ from measuring σ_{xz}, s_x, and s_z on an india-rubber slab with, I presume, $\eta = \frac{1}{2}$. Their values for these stresses therefore ought to satisfy (i) and (iv); it is difficult to understand how Mr. Martin can believe them to satisfy (i) and (iii) with $\eta = \frac{1}{4}$, which is needful in the case of the masonry dam.

To Sir Oliver Lodge I can only reply:—"Read our memoir and you will find that the influence of the weight of the dam is fully discussed, and experimentally determined. You will also find that the actual *tearing* was only reached by 'excessive water pressure.'"

With regard to Prof. Brown's views on the effect of the terminals, the following words are used to limit the actual theory applied:—"Let us suppose an indefinitely long dam, or if it be of finite length that its terminals abut against rigid supports." The words *abut against* were purposely used instead of *are built-in* to mark what conditions were being assumed. It is quite another point how far my theoretical dam may be considered an approximation to a real dam. Prof. Brown, emphasising the variability of conditions in any practical case, says that engineers "have used a simple but approximate method of estimating stresses in a dam, based on the flexure of beams." My criticism is that this is not an approximation at all, and that if it were they have neglected to apply their own conditions to the vertical sections, in which case they would have found any existing dam so lacking in stability that they would have promptly dropped a theory which, if carried to its logical conclusion, condemns all existing dams! Now, when an engineer treats a dam as a beam, he forgets two points:—(1) That the length of a beam must be large compared to the linear dimensions of the cross-section. In the case of the dam, its base, the built-in cross-section, is of the same dimension as to depth as the length of the beam, *i.e.* the height of the dam. The ordinary formulæ of flexure are thus absolutely inapplicable, as anyone who knows de Saint-Venant's classical memoir on flexure will at once realise. (2) That the faces of the beam must be free to expand in order that the theory may apply. In applying his theory of flexure, therefore, the engineer, so far from building-in his terminals, does not even suppose them to abut! I contend that if Prof. Brown's balcony were 100 feet broad, but *half a mile or a mile long*, he would find little difference in the stresses except relatively close to the abutments, whether he supposed the terminals abutted or were built-in. In any case, I am convinced on both theoretical and experimental grounds that for the bulk of the dam far better results will be obtained by applying the uniplanar equations of stress than by any attempt to deal with a solid practically as broad as it is long by a theory of flexure.

That the terminal conditions produce effects goes without saying; I agree with Prof. Brown that we should, if possible, consider them, but their order of importance is that of two terminal supports to a balcony, say 1 foot broad, 1 foot deep at the built-in edge, uniformly loaded, and perhaps 50 feet long. Such effects are purely secondary, and it will be time enough to consider them when we have reached some agreement as to the main stresses; these stresses can be fairly reached by taking, as I have done, an indefinitely long straight dam *abutting* against rigid terminals.

With most of Prof. Brown's statements in his appreciative criticism I am in complete harmony. No theory will replace local knowledge—that of the geologist as well as that of the engineer—no theory will perhaps be of equal value with past experience, but it can be helpful and even

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suggestive. Mr. Pollard and I did not smooth our observations, as has been done in some other cases, because it was desirable to show how much variability is due to local conditions, how much to observational error. Much of what Prof. Brown would attribute to want of homogeneity in our material I know to be due to the changes in external contour, and this is one of the urgent problems of dam construction. Our experimental work shows that very large changes are made by modification of the contour. Is it not possible to reach experimentally better forms for the flank of a dam than are at present in use?

The cost of experimental work on a suitable scale is prohibitive to the individual experimenter, but it appears to me that something on a really big scale ought to be done by the Institution of Civil Engineers. The problem is a very important one, and I do not believe, notwithstanding what I have read to the contrary, that the keen mathematician feels anything but intense awe in the presence of the creative engineer, who has succeeded, notwithstanding the weak-kneed theory provided by men of science, in producing such monumental works as the Assuan and Vyrnwy dams by the force of mechanical appreciation alone.

KARL PEARSON.

The Nature of γ and X-Rays.

IN a letter to NATURE of October 31, 1907 (vol. lxxvi., p. 661), a copy of which arrived here recently, Mr. Barkla criticises a paper of mine which was published in the *Philosophical Magazine* for October. In that paper I tried to show how closely the properties of γ and X-rays were in agreement with the hypothesis that they consisted mainly at least of neutral pairs, and I pointed out that even the peculiar polarisation effects which Mr. Barkla had shown to exist might be explained, not unreasonably, as a consequence of the rotatory motion which such pairs would probably possess. I suggested that a pair might be more likely to become entangled with and deflected by an atom revolving in the same plane as itself.

Mr. Barkla describes an experiment in which he has measured the amount of scattering of X-rays in different directions, and compares his results with those which he expects to obtain as the result of calculations based on each of the two hypotheses in turn, that of the neutral pair and that of the ether pulse. He states the results to be against the former theory.

But for the result from his calculations in the case of the neutral pair, he makes the assumption that the probable direction of motion of a neutral pair on emergence from an atom with which it has been entangled is independent of its original direction of motion.

There is no justification for this assumption. It does not even appear to be probable. Consequently, the experiment has no value as a critical test. Yet I fully agree with Mr. Barkla that the dependence of the amount of secondary radiation upon the angle which its line of motion makes with that of the primary ray is a very proper subject of study; it might be expected to furnish much-wanted information.

For this reason I also have been investigating one aspect of this question. With the assistance of Mr. J. P. V Madsen, of this University, I have been comparing the secondary radiations issuing from the two sides of a plate through which γ rays are passing. On the ether pulse theory there should be complete symmetry, provided that the rays have not been appreciably absorbed on their way through. Secondary radiation, whether material or not, originated in an atom by a passing pulse, is just as likely to go forwards as backwards. This is, indeed, always assumed by writers on the ether pulse theory, *e.g.* by Mr. Barkla himself in the letter already referred to.

On the other hand, if the γ rays are material, it is quite possible, though not necessary, that the secondary radiations on the two sides of the plate should be different.

As a matter of fact, there is the most remarkable want of symmetry, and this is fatal to the ether pulse theory of the γ rays. Moreover, all our experiments so far show that, on the whole, the kathode radiations from a given stratum of matter traversed by γ rays possess momentum in the original direction of motion of the rays, and this shows that the rays are material.

The experiments are very simple, and are not wholly new. The secondary kathode radiations due to a stream of rays impinging on a plate have been studied by many observers, who all concur in the statement that they increase with the atomic weight of the material of the radiating plate. For example, the figures for Pb, Al, and C are about as 100 to 30 to 15. In fact, they follow almost the same law as in the case of β particles. The reason for this will appear presently. The secondary kathode radiations that appear where γ rays *emerge* from a plate have been less studied, but Eve has shown that they consist largely of kathode particles, and Dawes (*Phys. Rev.*, xx., p. 182) that they do not appear to follow the same law as what may be called the "incidence" radiations. Further, Wigger has made the very important observation ("Jahrbuch der Radioaktivität," Bd. ii., pp. 428–430) that in certain circumstances the γ rays issuing from a plate of Al which they have traversed make *more* secondary rays than when they issue from Pb.

All these facts may be conveniently studied together. Let an ionisation chamber be made of cylindrical form with plane ends, and let a pencil of γ rays be directed along the axis. Let the rays first pass through a cm. or so of lead. Let them then pass through a pair of plates which can be inverted; a convenient pair may be made of a lead plate 1 mm. and a carbon plate 1 cm. thick; these are to form one end of the chamber. It will then be found that there is more current through the chamber when the C plate is next to it, and the "emergence" secondary rays are produced in the carbon, than when the Pb plate is next the chamber. But if the plate closing the other end of the chamber be at one time Pb, at another time C, the reverse effect occurs. At this plate "incidence" secondary rays are produced by the same pencil of γ rays, and these are greater when the plate which is struck is Pb than when it is C. The differences are of the order of 10 per cent., 20 per cent., up to 60 per cent., depending on the circumstances of the experiment. The materials and the form of chamber may be largely varied, but the same want of symmetry is always there.

It appears that the "emergence" radiations from the plate through which the γ rays have entered are more important than the "incidence" radiations from the other plate. The latter serves mainly as a reflector or scatterer of the rays from the former, and this is the reason why there is less current when it is formed of a material of smaller atomic weight, following the usual rule for β rays. This effect is a general one, and it serves to explain why all observers have found less kathode radiation due to γ rays from Al or C than from Pb, when actually the rays produce more from the former than the latter when they have been first sifted by a cm. or so of lead. A stream of γ rays contains β particles derived from the γ rays by the influence of the last substance traversed. It is the scattered portion of these which constitutes the main portion of the secondary radiations due to γ rays, and the reason why the incidence radiations run parallel to those of β rays is obvious. Nevertheless, there are small variations due to secondary rays formed in the material of the plate itself, the quantity of which is influenced by the nature, not only of this material, but of the screens through which the rays have previously passed. This is because the γ rays are heterogeneous, as first shown by Kleeman. It is when the rays have passed through some thickness of lead that they are acted on with greater effect by Al or C than by lead. The quantity of kathode radiation set free in the radiator itself depends on the quality of the rays as well as on the radiating material. The particles at first move mainly, perhaps entirely, in the original direction of motion of the γ rays, but are subsequently scattered, and contribute to some extent to the "incidence" secondary radiation. But the principal portion of the incidence radiation is due to β particles which were in the stream of γ rays before it struck the plate.

It would make this letter too long if I were to discuss these results with any fulness, or to show their relations to results already obtained. I hope to publish a fuller account in a short time. Meanwhile, I will point out that the experimental proof of the material nature of the γ rays carries with it, almost surely, a corresponding proof as regards the X-rays. The points of similarity are too numerous for it to be otherwise. Only, as I have said in

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